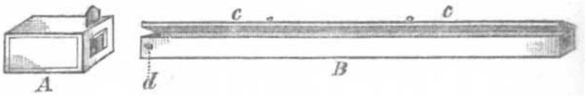
scarcely say that I anticipated it in its present form), that I would willingly have consented to giving him away *gratis* if an opportunity had offered. Probably few of us admired the animal more than I did; but I have considerable knowledge of those who have attempted to keep such elephants in captivity in Europe. It is said that, as we have no difficulty in keeping lions, which are more dangerous, there need be no danger with elephants; but the deduction is not sound. A lion is always dangerous, and can be treated accordingly; an elephant, which inspires confidence by its usual docility, is on that very account a far more difficult and dangerous animal to deal with. In many zoological gardens on the Continent I have seen elephants boxed and chained up, without being allowed to take a foot of exercise, sometimes for years together; and on inquiry I have always found that it had been necessary to restrain the animal, because at some unexpected moment it had killed or injured its keeper. In India this would only be looked upon as an ordinary incident in an elephant's life; but if such an event were to happen in our Gardens (as I must say I have felt morally certain it would do sooner or later, if Jumbo remained there), what should we have had to do with the animal? Could we have ever again let him pace about the Gardens with his precious freight of little children on his back? But much worse than even killing a single keeper might have happened if the animal had once got beyond control. We have been warned by high legal authority of our responsibilities on this subject. It is possible that we may have been *too* apprehensive, *too* careful, about the lives of our servants and of our visitors; we may possibly have looked at difficulties incident to the management of our gardens, into the details of which it would be useless to detain this meeting by entering upon, in too serious a light; but this was a case in which we felt that to be on the safe side was the right course to pursue. I do not say that other bolder and more enterprising managers, who might look upon the attractions of the Gardens in a more commercial spirit, might not possibly have taken a different course; for we were quite aware that the loss of the animal might for a time be detrimental to the income of the Society. For this reason we also, as custodians of the Society's finances, thought it not right to decline to avail ourselves of the very unexpected opportunity of diminishing that loss, as far as possible, by the animal's sale. Some persons have called in question the "morality" of this transaction. How any one who has ever sold a horse, cow, sheep, or pig can do so, I cannot imagine. If the purchasers elect to take an animal, knowing all its imperfections, and the vendors are satisfied that it will pass into hands where there is every reasonable prospect of its being properly treated, what more can be required? Then we have been told that we ought to have killed the elephant. To this I decidedly demur, unless the principle is admitted that every one who has a horse, a dog, or any other animal, which has become through any circumstances inconvenient for him to keep, is bound to destroy it. This may be the doctrine of a few visionary enthusiasts, but it is not common sense, it is not humanity. If the life of an animal is of any use to it (and I see no reason why this elephant may not enjoy his life for perhaps a few years longer), there is no reason for taking it away until the time comes when it is absolutely necessary to do so. Besides, as I mentioned before, as trustees and managers of the Society's property, we are bound to look after its finances. You surely all know that the operations of the Society cannot be carried on without means, and that every penny received by the Society is spent upon the purposes indicated in the Charter; and yet many persons (I am almost ashamed to allude to such folly and ignorance), have spoken as if the Council, or the officers of the Society, had some direct pecuniary interest in selling the elephant. This brings me, in conclusion, to the one most serious side among the many ludicrous incidents that have arisen out of this affair. This is the rash or wilful misrepresentations that have been so freely indulged in against a body of gentlemen of whose general qualifications for the offices which they hold it is not perhaps necessary for me to speak in this assembly (their names should be a sufficient guarantee of this), but of whom I may say, from my intimate knowledge, that they are constantly endeavouring, often at considerable personal sacrifice, to bring their varied knowledge and experience to bear upon carrying out the work of the Society for the advancement of science, and for the benefit of the Fellows of the Society and the public generally. Our accomplished Secretary of whose successful general administration of the Society no one who did not know its condition as I happen to do before he took office, and has not watched its growing prosperity for the last five and twenty years, can form an adequate idea, has not been spared, although in his share in this transaction he certainly had no interest but that of the Society at heart.

There is much in this which is to me a novel and painful experience; but I am told that it is what all must expect who undertake the responsibility of any kind of work for the benefit of others. However this may be in political life, it might have been hoped that among those who followed the calmer pursuits encouraged by this Society, there would not have been any found who, either openly or under cover of anonymous slander in newspaper articles, letters, and postcards, would have imputed to us, which I regret to say has been so freely done, motives absolutely contrary to those by which we know we have been ever actuated.

ON DUST-EXPLOSIONS IN COLLIERIES

THE observations and experiments of M. Vital, in France, and of Mr. Galloway, Prof. Abel, and the late Prof. Freire Marreco, in this country, have shown, beyond all question, that we must look to the power possessed by coal-dust, and possibly even by finely-divided incombustible inorganic matter, when suspended in air, of propagating or enlarging the area of an explosion as one of the main causes of those frightful occurrences, which now and then decimate even an entire mining community. There can be little doubt, that so far as the loss of life is concerned, dust-explosions are, as a rule, far more disastrous than mere explosions of gas. A shot is blown out, or, by some mischance, the gas in the goaf, or in some hole in the roof, is fired: the concussion of air raises a cloud of dust, among the particles of which the flame rushes with explosive violence. Fresh dust is raised, to form fresh fuel for the devouring flame, which. as in the case of the Penygraig explosion, so carefully investigated by Mr. Galloway, is thereby enabled to penetrate and search into the innermost recesses of the workings, provided they be sufficiently dry and dusty. Every particle of free oxygen is thus practically used up, and the resultant atmosphere is a suffocating mixture of nitrogen, carbonic acid, carbonic oxide, hydrocarbons, and partially-coked dust, against which the men, over whom the flame may have passed, with little hurt, have not the slightest chance.



It may possibly be of interest to those who, like myself, have to teach chemistry in a coal-mining district, to know of an experiment which illustrates in a striking manner the main features of a dust explosion. The experiment is to make an explosion at one end of a long and narrow wooden tube, representing the gallery of a mine; to show

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that the concussion will raise a cloud of finely divided solid matter from the bottom of the tube along which the flame will be propagated and be driven out at the other end. *A* (see figure) is a wooden box 12 inches long, 8 broad, and 6 deep, closed on all sides, with the exception of a rectangular hole ($3\frac{1}{4} \times 2\frac{3}{4}$ inches), into which can be inserted a long narrow rectangular tube (B), also of wood, which may be 20 feet or more in length; the upper side (*c c*) of this tube is hinged, and along the bottom is strewed a thin layer of finely-divided dry coal-dust, or, what is better in the lecture-room, lycopodium powder. Into the wooden box, which in my apparatus has a cubic content of more than a gallon (5 litres) is placed about $1\frac{3}{4}$ pints (say 1 litre) of coal-gas; this can be most readily effected by pouring this amount of water into the box and displacing it over the water-trough by a current of the coal-gas. The opening is then closed by a sliding lid, and the gaseous contents are mixed by violently shaking the box for a minute or so. The end of the long tube (along which the powder or dust has been strewn, and the lid *c c* pushed down) is then inserted into the box, and the gaseous mixture is fired by thrusting a lighted taper through a small hole (*d*) at the end just where the tube enters the box. The mixture of coal-gas and air explodes, and the flame rushes along the whole length of the tube with astonishing velocity, and is driven, often to a distance of six or seven feet, out at the other end, and is followed by a cloud of smoke.

The experiment is unaccompanied by danger, and is so simple that it may be readily performed in a lecture-room. I showed it some time since to a number of colliers and others engaged in coal-mining, and it seemed to bring home to them far more forcibly than possibly any amount of mere description would have done, the real character of the phenomenon. T. E. THORPE

THE PHOTOGRAPHIC SPECTRUM OF THE GREAT NEBULA IN ORION[1]

LAST evening (March 7) I succeeded in obtaining a photograph of the spectrum of the great nebula in Orion, extending from a little below F to beyond M in the ultra-violet.

The same spectroscope and special arrangements, attached to the 18-inch Cassegrain telescope with metallic speculum belonging to the Royal Society, were employed which have been described in my paper on "The Photographic Spectra of Stars" (*Phil. Trans.*, 1880, p. 672).

The exposure was limited by the coming up of clouds to forty-five minutes. The opening of the slit was made wider than during my work on the stars.

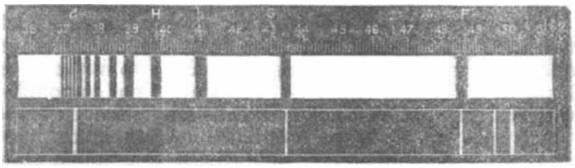
The photographic plate shows a spectrum of bright lines, and also a narrower continuous spectrum which I think must be due to stellar light. The bright stars forming the trapezium in the "fish's mouth" of the nebula were kept close to the side of the slit, so that the light from the adjacent brightest part of the nebula might enter the slit.

Outside this stronger continuous spectrum I suspect an exceedingly faint trace of a continuous spectrum. In the diagram which accompanies this paper the spectrum of bright lines only is shown, which is certainly due to the light of the nebula.

In my papers on the visible spectrum of the nebula in Orion, and other nebulæ (*Phil. Trans.*, 1864, p. 437, and 1868, p. 540; also *Proc.* Roy. Soc., 1865, p. 39, and 1872, p. 380), I found four bright lines. The brightest line, wave-length 5005, is coincident with the less refrangible component of the double line which is strongest in the spectrum of nitrogen. The second line has a wave-length of 4957 on Ångström's scale. The other two lines are coincident with two lines of hydrogen, Hβ or F, and Hγ near G.

In the photograph, these lines which had been observed in the visible spectrum are faint, but can be satisfactorily recognised and measured. In addition to these known lines, the photograph shows a relatively strong line in the ultra-violet, which has a wave-length 3730, or nearly so. The wide slit does not permit of quite the same accuracy of determination of position as was possible in the case of the spectra of stars. For the same reason, I cannot be certain whether this new line is really single, or is double or multiple. In the diagram the line is represented broad, to indicate its relative great intensity.

This line appears to correspond to ζ of the typical spectrum of white stars (*Phil. Trans.*, 1880, p. 677). In these stars the line is less strong than the hydrogen line near G; but in the nebula, it is much more intense than Hγ. In the nebula, the hydrogen lines F and Hγ are thin and defined, while in the white stars they are broad, and winged at the edges. The typical spectrum has been added, for the sake of comparison, to the diagram.



I cannot say positively, that the lines of hydrogen between Hγ and the line at 3730 are absent. If they exist in the spectrum of the nebula, they must be relatively very feeble. I suspect, indeed, some very faint lines at this part of the spectrum, and possibly beyond λ 3730, but I am not certain of their presence. I hope, by longer exposures and with more sensitive plates, to obtain information on this and other points. It is, perhaps, not too much to hope, that the further knowledge of the spectrum of the nebulæ afforded us by photography, may lead, by the help of terrestrial experiments, to more definite information as to the state of things existing in those bodies.

THE ACTION OF CARBONATE OF AMMONIA ON THE ROOTS OF CERTAIN PLANTS, AND ON CHLOROPHYLL BODIES[1]

1. *Roots.*

THE observations which led to the first of these papers were originally made many years ago on *Euphorbia peplus*, and have now been extended to other genera. A plant of *E. peplus* having been dug up and carefully washed, the smaller rootlets may be placed under the microscope without further preparation, the thicker roots may be examined by means of sections. If such roots are left, before being examined, in a solution of carbonate of ammonia (1 to 7 per 1000) for a short time (varying from a few minutes to several hours), they present a wonderfully changed appearance. The most striking alteration is that the surface of the root assumes a *longitudinally striped appearance*, due to longitudinal rows of darker brown cells, alternating with lighter coloured rows. The darker colour is seen under a high power to be due to the presence of innumerable rounded granules of a brown tint, which the lighter-coloured cells are without. Similar brown granules are deposited in cells scattered throughout the parenchyma, and markedly in the elongated endoderm cells surrounding the vascular bundle.

The granules are apparently neither resinous nor fatty, for they are not removed by alcohol or ether; they are,

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[1] Paper read at the Royal Society, March 16, by William Huggins, D.C.L., LL.D., F.R.S.

[1] Abstract by Mr. Francis Darwin of two papers by Mr. Charles Darwin read before the Linnean Society on March 16.

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